

Synthesis Report: Census 2000 Alternative Questionnaire Experiment

FINAL REPORT

This research paper reports the results of research and analysis undertaken by the U.S. Census Bureau. It is part of a broad program, the Census 2000 Testing, Experimentation, and Evaluation (TXE) Program, designed to assess Census 2000 and to inform 2010 Census planning. Findings from the Census 2000 TXE Program reports are integrated into topic reports that provide context and background for broader interpretation of results.

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Executive Summary

This report summarizes the findings of three experiments included in the Census 2000 Alternative Questionnaire Experiment. The purposes of these experiments were diverse:

- **The skip instruction experiment** examined respondent performance in following skip instructions in the Census long form. It compared different ways of aiding respondents' navigation through the questionnaire. One design incorporated instructions and visual features to help respondents prevent errors before they occurred, and another was designed to help respondents detect errors after they occurred. In addition to these *prevention* and *detection* designs, other potential design improvements, such as using reverse print to attract respondents' attention to instructions, and rewording the standard "skip" instruction, were also tested.
- **The residence instructions experiment** involved the presentation of residence rules in Census 2000 short form. This research aimed to improve within-household coverage by rewording the roster instructions to be understandable to respondents, encouraging respondents to read them through appropriate placement and formatting, and by presenting the instructions so as to increase respondents' willingness to follow them.
- **The race and Hispanic origin experiment** compared the 1990-style race and Hispanic origin questions with the new questions in Census 2000 short form. It examined the effects of changes mandated by the Office of Management and Budget to allow the reporting of more than one race and reverse the sequence of the race and Hispanic origin items. Other changes in format, categories and wording were also introduced in Census 2000, and the net effects of all the changes on race and Hispanic reporting were analyzed.

All three experiments were limited to the mailout-mailback universe.

The results of the three experiments include the following:

- **Skip instruction experiment:** Errors of commission (which occur when respondents incorrectly answer questions they should have skipped) were significantly reduced in all of the experimental treatments, suggesting that the design changes improved respondents' perception and comprehension of the instruction. Errors of omission (which occur when respondents skip questions they should have answered) decreased for the Detection Treatment, but significantly increased for every other experimental treatment. Either type of error indicates respondent difficulty navigating the questionnaire, but their impact is different. Errors of omission result in missing data. Errors of commission increase respondent burden and frustration. **We recommend that the Census Bureau adopt the Detection method in mail questionnaires, since it significantly reduces both types of errors.**

- **Residence instructions experiment.** The changes in format, presentation, and wording of the residence instructions resulted in a significantly higher response to the household count question (which serves as an important indicator of missing data and flags large household follow-up.) The experimental panel also produced significantly fewer omissions among Hispanics in the low coverage stratum. **We recommend additional testing of the graphical and wording changes that led to these improvements, to better understand their effects and to further improve the quality of household count data.**
- **Race and Hispanic origin experiment.** Overall, the questionnaire revisions substantially improved the completeness of race and Hispanic origin reporting in mail short form questionnaires. In addition, Hispanics were less likely to report their race as Some other race, and more likely to report as White, in the 2000-style questionnaires.

Although there were no apparent questionnaire effects on the fraction reported as Hispanic, there were effects on the reporting of detailed Hispanic origin groups. The 1990-style questionnaire obtained more detailed reports of Hispanic origin than the 2000-style questionnaire, probably due to the effects of question wording differences as well as examples. Unexpectedly, there were three times as many reports of the example groups for Asian and Pacific Islander groups in the 2000-style form, which did not list examples, as there were in the 1990-style form, where examples were listed. The experiment demonstrates that some questionnaire design changes made in Census 2000 resulted in substantial improvements in data quality, but that other changes had unintended consequences. **We recommend careful pretesting and field testing of all changes to the questionnaire, and that similar but larger replication studies be conducted in future censuses to evaluate the effects of questionnaire changes on the comparability of data from one census to the next.**

The results of all three experiments point to interactions between question format and content, suggesting that we must attend to the complex relationships between format and meaning in self-administered questionnaires. These factors have been demonstrated here to have measurable effects on the data. These experiments demonstrate that format affects performance on branching instructions, affects response/nonresponse on the household count question and indirectly affects coverage and that format differences between 1990-style and Census 2000 forms affect race and ethnicity reporting.

1. Introduction

An Alternative Questionnaire Experiment (AQE) to test the effects of variations in the design of census mail questionnaires upon response rates and data quality has been conducted in each of the past three decennial censuses. The 1980 Alternative Questionnaires Experiment tested two FOSDIC matrix style forms, and a non-FOSDIC form that was intended to be attractive and easily understood (Mockovak 1984). The 1990 AQE experimentally compared five long form questionnaires that involved successively more radical departures from the traditional design, including wording and format changes, a kit containing individual questionnaires for each household member, and anonymous census questionnaires (Bates 1991, 1992).

The 2000 AQE incorporates three separate experiments, one involving census long forms and the other two involving short forms, with different objectives. Two experiments look forward, seeking improvements in the design of mail forms that may lead to improved data quality in the next census. A third looks backward, replicating the 1990 short form in Census 2000 in order to document the effects on the data of changes in the design of the Census 2000 short form.

All three experiments test combinations or “packages” of design features, rather than testing each design change separately in a controlled fashion that would permit inferences about their individual effects. Thus, firm conclusions can only be drawn about the combined effect of multiple design features, and this is an important limitation of all three experiments. However, previous research and testing often provides insights into the effects of particular design features. Another limitation is that the findings are only generalizable to the mailout/mailback universe. This excludes certain populations of interest, such as Indians living on reservations and Alaska Natives (of interest to the analysis of questionnaire effects on race reporting) or rural populations not enumerated by mail (of interest to the analysis of roster completeness).

The three experiments are:

1.1. Experiment A: *Effects of Altering The Design of Branching Instructions On Navigational Performance in Census 2000*, by Cleo Redline, Don Dillman, Aref Dajani, and Mary Ann Scaggs.

This experiment took as its starting point the difficulty many respondents have navigating the census long form, causing them to mistakenly skip questions they are supposed to answer or answer questions they are supposed to skip. Hypotheses derived from research on visual perception were applied to develop new strategies for helping respondents navigate their way through the questionnaire. One design incorporated instructions and visual features to help respondents prevent errors before they occurred, and another was designed to help respondents detect errors after they occurred. In addition to these *prevention* and *detection* designs, other potential design improvements, such as using reverse print to attract respondents’ attention to instructions, and rewording the standard “skip” instruction to clarify it, were also tested. The larger purpose of this experiment was to develop a better understanding and general principles of

how graphical design features of a questionnaire influence respondents' ability to navigate through it.

1.2. Experiment B: *An Experiment to Improve Coverage Through Revised Roster Instructions*, by Eleanor Gerber, Aref Dajani, and Mary Ann Scaggs.

This experiment focused on the problems respondents have filling out household rosters correctly. Erroneously including a person who does not live in a household, or omitting one who does, result in census coverage errors. The problem is exacerbated by counter-intuitive and complex census residence rules, which are often ignored by respondents who may have their own firm ideas about who belongs in their household. In addition, census residence rules do not follow any simple logic which is easily communicated to respondents. This research aimed to improve within-household coverage by improving the roster instructions in three ways: first, by rewording them to be understandable to respondents, second by encouraging respondents to read them through appropriate placement and formatting, and third, by presenting the instructions so as to increase respondents' willingness to follow them.

1.3. Experiment C: *Questionnaire Effects on Reporting of Race and Hispanic Origin: Results of a Replication of the 1990 Mail Short Form in Census 2000*, by Elizabeth Martin.

This experiment replicated a 1990-style mail short form during Census 2000 and compared the results to data from Census 2000 short forms in order to evaluate how the questionnaire changes affected reporting of race and Hispanic origin reporting. The questionnaire changes introduced in Census 2000 included allowing the reporting of more than one race and reversing the sequence of the race and Hispanic origin items, as well as other changes in format, categories and wording.

This synthesis report summarizes the results of the three experiments, and seeks to draw more general conclusions from them. Section 2 describes the methods used in the AQE, and section 3 summarizes the major findings of each separate experiment (as well as additional analysis of one that did not appear in the final report). More detailed discussions of the methods and findings of each experiment are found in their respective final reports.

2. Methods

2.1. Questionnaire Development

Experiment A. Conventional branching (skip) instructions may easily be overlooked. Alternative forms were designed to manipulate verbal (revising “skip to” to “go to”) and graphic (reverse print) features, and to implement error *detection* or error *prevention* strategies. Five alternative forms were developed and tested in a classroom experiment and in cognitive tests. The testing was used to revamp and refine the designs which were tested in the AQE. The questions were chosen specifically so that content would not provide cues to navigation. Each

panel used identical content, ensuring that the length of each panel was the same. See Fig. 1 for the five formats that were included in the AQE.

Experiment B. Several different roster formats were designed, and two rounds of cognitive testing were conducted to select and refine the best format, which became experimental panel 7 (see chart 1, below) in this experiment. The experimental roster included several design features, including placing the instructions before the roster question, rewording the instruction to read “our guidelines” before answering the question, enclosing the instructions and answer box in a black outlined box with a slightly brighter background than the surrounding questionnaire, double-banking the include/exclude rules and rewording them to make them more inclusive and easier to understand (see Fig. 2 for the Census 2000 roster question and Fig. 3 for the experimental version).

Experiment C. A 1990-style form was developed which preserved 1990 question wording, categories, order, type size, matrix format, and other essential design features which might influence responses. The 1990-style form was not identical to the 1990 census form, however, because it changed dates and incorporated several publicized and familiar features of the 2000 design (color, logo, “Start here” instruction, envelope and letter). Any questions not included in the Census 2000 short form, such as marital status, were dropped. The form was not pretested. See Figs. 4 and 5 for the two versions of the race and Hispanic questions which were compared.

2.2. Sample Design

The AQE included eight panels of approximately 5,000 households (10,000 for one panel) each. The sample was stratified into high coverage areas (HCAs) which had low proportions of minorities and renters in the 1990 census, and low coverage areas (LCAs) with high proportions of minorities and renters. Sample cases were distributed equally between strata, implying that households in LCAs were oversampled. All results are weighted to reflect sampling probabilities.

Addresses on the Decennial Master Address File in the mailout/mailback areas of the country at the time sample selection took place served as the universe for sample selection (Woltman, 1999). Addresses in non-mailback areas (mostly rural areas) were excluded from sample. This excludes certain population groups of interest, including American Indians living on reservations and Alaska Natives. Addresses that were added later as a result of coverage improvement operations were excluded, as were addresses in the sample for the Accuracy and Coverage Evaluation survey. A systematic sample by state, stratum, and treatment was selected.

2.3. Experimental Treatments

The AQE included eight panels (three short form and five long form panels). Chart 1 shows the experimental and control panels and number of households in each. Letters in parentheses indicate whether the panel is part of Experiment A, B, or C of the AQE.

Chart 1. Alternative Questionnaire Experiment Panels

Panel	Mailing sample size	Response rate
Long form panels		
1. <u>Census 2000 long form questionnaire</u> (Control panel, experiment A)	5,257	63.95
2. <u>“Go to” questionnaire</u> (A): identical to panel 1 questionnaire, except “skip to” is replaced with “go to” throughout.	5,248	64.34
3. <u>Reverse print questionnaire</u> (A): identical to panel 2 questionnaire, except the “go to” instruction appears in reverse print (yellow letters on black background).	5,251	61.82
4. <u>Prevention questionnaire</u> (A): adds an instruction alerting respondents to look for branching instructions; adds arrows and other features to attract attention to instructions.	5,241	63.13
5. <u>Detection questionnaire</u> (A): adds an arrow to guide respondents away from branching instruction when appropriate; adds information to the next question to allow respondents to judge if they have correctly skipped.	5,238	63.25
Short form panels		
6. <u>Census 2000 short form questionnaire</u> (Control panel, experiments B, C)	5,252	73.07
7. <u>Revised roster questionnaire</u> (B)	5,256	73.52
8. <u>1990-style short form questionnaire</u> (C): replicates 1990 question wording, categories, order, type size, matrix format, and other essential design features. Incorporates Census 2000 color, logo, envelope and letter. Drops questions not included in the Census 2000 short form (marital status)	10,499	72.60

For all panels, questionnaires were mailed out according to the Census 2000 schedule, with every sampled address mailed an advance letter, a questionnaire, and a follow-up postcard. For respondents in the AQE, the responses provided on the mail forms were their census data. Telephone Questionnaire Assistance operators were trained to answer questions from respondents who received an experimental questionnaire (e.g., about the instruction in the 1990-style form to select one race category from respondents who wanted to report more than one).

Questionnaires were mailed back to the National Processing Center in Jeffersonville, Indiana, where they were keyed and processed separately from production Census 2000 data, which were imaged. Households which did not return a mail questionnaire were followed up as part of the Census 2000 nonresponse operation and are not included in this analysis. Response rates for the eight panels are shown in the second column of Chart 1. The rates are weighted and exclude undeliverable addresses, duplicate forms, and blank forms. Response rates for the two experimental short form panels do not significantly differ from the control². As was true in the census, response rates for the long form panels are significantly lower than for the short form panels. There are no significant response rate differences among the long form panels. (Significances were calculated using Bonferroni adjustments for multiple comparisons; see Dajani and Scaggs, 2001.)

2.4. Additional Processing and Sources of Data

Experiment B. To analyze the effects of roster variations, coverage was measured by a specially developed telephone coverage reinterview, conducted by Westat about four months after the census, in late July of 2000. The reinterview sample consisted of cases that had completed and returned the census form, had phone numbers, and were not sent to large household follow-up. These cases were subsampled randomly at a rate of 50 percent in the high coverage area stratum. The total sample size for the reinterview was 4,218 households. There were 2,958 completed interviews: 1,497 completed cases in the control and 1,461 in the experiment. This represents a response rate of 70.35 percent in the control and 69.90 percent in the experiment, with an overall response rate of approximately 70 percent.

Experiment C. To increase sample size and improve reliability for the analysis of questionnaire effects on race and Hispanic reporting, the short form control (panel 6) was supplemented with mail returns from the control panel for the Response Mode and Incentives Experiment (RMIE) (Guarino, 2001). The RMIE control group sample of approximately 20,000 addresses was selected from the same universe using the same stratification as AQE, except the sample was allocated proportionately to the HCA and LCA strata. All addresses in the RMIE control group received Census 2000 short form questionnaires, which were processed separately, like the AQE. The response rate was 71.5 percent.

Race and Hispanic origin data for panels 6 and 8 (and the RMIE control data) were coded and pre-edited by applying a simplified version of pre-edits used in Census 2000 production. (See

²Although overall short form response rates do not differ, panel 8 has a significantly lower response rate (57.63 percent) than the control panel 6 (60.78 percent) in the low coverage area stratum ($p < .05$). This difference suggests that the Census Bureau's investment in developing a "user-friendly" design for the Census 2000 short form did achieve a higher response rate in these areas, compared to a 1990-style form.

Martin 2002 for details.) Missing data were not imputed or allocated, as they would be in fully edited census data. Results may differ for fully edited census race and Hispanic origin data.

3. Major Findings

3.1. *Effects of Altering the Design of Branching Instructions On Navigational Performance*

The analysis examined two types of errors. Commission errors occur when respondents incorrectly provide an answer to questions they should have skipped, and omission errors occur when respondents skip questions they should have answered. Either type of error indicates respondent difficulty navigating the questionnaire, but their impact is different. Errors of omission result in missing data. Errors of commission increase respondent burden and frustration.

To control for differences in the number of questions that respondents answered, analysis was limited to Person 1 responses (the questionnaire provides space for data to be provided for up to six household members). Error rates were calculated for questions with branching instructions (because only their designs differed between form types) and with valid responses (because only then was it evident whether a respondent should branch or not).

Error Rates For All Census Long-Form Items With Branching Instructions (Table 1)

Instruction Treatment	Errors of commission	Errors of omission
1. Census 2000 (Skip To instruction)	19.7%	5.0%
2. Go To Control	20.8	5.4
3. (Go To) Reverse Print	17.9	7.6
4. (Go To) Prevention	14.7	7.0
5. (Go To) Detection	13.5	4.0
Statistical Comparison		
1 vs. 2	n.s.	n.s.
2 vs. 3	p < .01	p < .01
2 vs. 4	p < .01	p < .01
2 vs. 5	p < .01	p < .01
3 vs. 4	p < .01	n.s.
3 vs. 5	p < .01	p < .01
4 vs. 5	n.s.	p < .01

Note: lack of statistical significance is denoted by “n.s.”

The results indicate that simply changing the wording of branching instructions from “skip to” to “go to” (panel 1 versus 2) did not significantly affect either errors of commission or omission. Probably respondents did not notice either instruction, and no amount of rewording will help if the problem is that respondents are not reading the information in the first place.

Panel 2 was adopted as the control for comparison with the remainder of the experimental panels, since all shared the “go to” instruction wording. All three experimental treatments resulted in significantly fewer errors of commission than panel 2. The reverse print instruction (panel 3) had significantly fewer errors than the instruction without reverse print (panel 2). The prevention questionnaire had significantly fewer errors than the reverse print instruction. There was no further significant reduction in commission errors with the detection instruction overall (although there was in the LCA stratum). The fact that the commission error rate decreases across the Go To, Reverse Print, Prevention and Detection Treatments suggests that the changes made from one design to the next improved respondents’ perception and comprehension of the instruction.

A different pattern occurs for errors of omission. While errors of omission decreased for the Detection Treatment, they significantly increased for every other experimental treatment. Possibly, the attention-getting features of the experimental treatment distracted respondents’ attention from other cues about which questions they were supposed to answer. The better performance of the detection method may be due to two of its features. First, it included a visual cue (arrow) to direct respondents to the next question. Second, the feedback mechanism (the left-hand arrow that came off the non-branching response option and pointed to a parenthetical feedback phrase) may have helped respondents avoid errors of omission.

Thus, commission errors were decreased in this treatment without the omission errors increasing. The level of omissions in the Detection treatment was significantly less than in the “Go to” control. Interestingly, some cognitive respondents and respondents in a debriefing expressed the opinion that the many arrows in the detection method were “confusing”—even though the results suggest these arrows improved their performance! This indicates a need for care about what evidence should be used in making decisions, and also indicates that perhaps the effects of the arrows need to be tested separately.

Wide variations existed in error rates for individual questions. The reasons were not explored in this research. However, the results suggest that respondents do not understand the questions or the underlying response task (that they need not answer every question). As a result the respondent burden is greater than necessary (on the average, respondents are answering 20 percent more questions than they need to). Further research is needed to improve respondents’ understanding of the questions and the response task to reduce respondent burden.

Thus, manipulating the verbal symbolic and graphic languages of branching instructions significantly influences whether the instructions are followed. Further research is needed into the ways that graphic features interact with reading comprehension—that is, what respondents read, the order in which they read it and their consequent interpretation of that they read. This is an

area of questionnaire design that is clearly emerging as critical to data quality and in need of further research.

3.2. *An Experiment to Improve Coverage Through Revised Roster Instructions*

The revised roster design was evaluated using several error measures: nonresponse for the household count box, where respondents recorded the number of persons in their household; omission rates; and erroneous enumeration rates. Omission rates were calculated as the number of Census Day residents omitted from the census roster (but identified during reinterview) divided by the number of correct enumerations, using information both from the census form and from the reinterview. Erroneous enumeration rates were calculated as the number of persons on the census roster who were identified as Census Day non-residents in reinterview, divided by the number of people on the census roster. In addition, the demographic characteristics of people enumerated using the redesigned roster and Census 2000 roster design were compared.

Item Nonresponse. One of the alterations in the experimental version of the form was the placement of the box where respondents were to record the number of persons in their households. It is critical that the box where respondents record the number of persons in their households be completed, since it serves to flag missing person-level data and to cue large household follow-up. Any increase in item nonresponse in this item would be unacceptable. The item nonresponse for this item is significantly lower in the experimental form (.80 percent) than in the control (1.78 percent) at the 0.01 level of significance. Thus, the item nonresponse rate was cut in half, and the difference was significant in both strata. This demonstrates that the wording changes and/or the format integrating the instructions with the first question were effective in getting respondents to fill in the box.

Omissions are persons who should have been listed on the census but were not. Such persons were identified only in the reinterview (since by definition, they were not on the Census form.)

There is no significant form difference in the omission rates, which were 1.13 percent for the control and 1.21 percent for the experimental form. However, the experimental form had a significantly lower omission rate for Hispanics in the low coverage stratum, as shown in Table 2.

Omission Rates for Hispanics by Panel and Strata (Table 2)

Panel	Stratum		
	Total	HCA	LCA
Control	3.54%	3.90%	3.23%
Experiment	2.55%	4.26%	1.00%
Total	3.00%	4.09%	2.02%
Control vs. Experiment: Statistical Comparison	Not Signif	Not Signif	p < .05

Erroneous enumerations are persons who were included on the census forms although they were not legitimate census day residents. They included persons who spent most of their time elsewhere, or who were in group quarters where they should have been counted on Census Day. Examples are college students living away from home and persons in the military stationed elsewhere.

No significant panel differences were found in erroneous enumeration rates, which were 0.40 percent for the control and 0.39 percent for the experimental form.

Although the rates did not differ by panel, the characteristics of the people who were erroneously enumerated did, for the low coverage stratum: in the experimental panel, the fraction who were young people 18-35 years old was 58 percent, compared to 30 percent in the control panel.

The reasons why the 57 people age 18 to 35 were erroneously enumerated were further examined. Table 3 shows that college students account for a substantial portion of the erroneous enumerations in both forms (and in both the LCA and HCA strata; this result is not shown). However, the largest number of erroneous enumerations is not accounted for by the categories of erroneous enumerations which are usually included in residence rules research, such as college, military, and various group quarters institutions. This suggests that highly mobile people account for a substantial proportion of erroneous enumerations. This confirms previously conducted qualitative research that has demonstrated that young adults in this age group may be highly mobile for reasons other than college.

Frequency of Reasons Given for Erroneous Enumerations of People Age 18 to 35 (Table 3)

	Total	Control	Experiment
College	23	9	14
Military	4	3	1
Institution (GQ)	2	0	2
Unknown	28	12	16
Total	57	24	33

Conclusions. The results suggest that the revisions were effective. We do not know which of the design and wording changes are responsible for the improvements. Further research is needed to better understand how the different design features affect responses, how respondents naturally read questionnaires, and the relationship between graphical presentation and meaning in them. We recommend additional research that builds on these encouraging findings in further cognitive and field tests of wording and graphical design changes in the roster questions and instructions.

Differences between the effectiveness of these techniques in high and low coverage areas are encouraging, since coverage improvements occurred for a group (Hispanics) with relatively high rates of omission in the census. The differences also suggest that demographic, cultural, or social

factors may influence the effectiveness of one or more of the graphical and wording changes. For instance, perhaps the new format was easier to understand and follow by respondents who speak Spanish rather than English as their native language, or by those who are less educated. The experiment does not allow us to identify the factors which may have played a role, but the results suggest that further research is needed to expand our understanding of their influence.

3.3. *Questionnaire Effects on Reporting of Race and Hispanic Origin*

Item nonresponse rates and differences in reporting of Hispanic origin and race were compared between the 1990- and 2000-style questionnaires and assessed using VPLX (Fay, 1998).

Item Nonresponse Rates. Overall, the questionnaire revisions substantially improved the completeness of race and Hispanic origin reporting in mail short form questionnaires. Item nonresponse (i.e. blank or uncodable responses) for Hispanic origin was 3.33 percent in 2000-style questionnaires, compared with 14.46 percent in 1990-style questionnaires, as seen in Table 4.

Reporting of Hispanic Origin in Mail Questionnaires, by Form Type (Table 4)

	Form type		$t_{2000-1990}$
	2000-style	1990-style	
TOTAL	100.00%	100.00%	
Hispanic	11.17	11.14	.05
Non-Hispanic	85.50	74.39	15.8*
Hispanic item blank, uncodable	3.33	14.46	-21.9*
N	40,723	16,616	

* $p < .05$

Item nonresponse for race was 3.27 percent for 2000-style forms and 5.95 percent for 1990-style questionnaires, as shown in Table 5. For Hispanics the reduction in race item nonresponse was very large, from 30.53 to 20.79 percent in the 2000-style questionnaires, but remained quite high.

Race Item Nonresponse Rates by Form Type and Hispanic Origin (Table 5)

Hispanic Origin	% of people missing data on race		
	2000-style	1990-style	t₂₀₀₀₋₁₉₉₀
Total	3.27%	5.95%	-7.34*
Hispanics	20.79%	30.53%	-4.42*
Non-Hispanics	.60%	1.53%	-5.03*
Hispanic origin missing	13.18%	9.72%	2.00*

*p<.05

Hispanic Origin Reporting. Results in Table 4 show that the same percentage (slightly over 11.1 percent) was reported as Hispanic in both 1990- and 2000-style forms, despite the reversed sequence of Hispanic origin and race and question wording differences. In past censuses, most people for whom origin was missing were non-Hispanic. On this assumption, these results suggest that questionnaire changes reduced item nonresponse but did not otherwise affect reporting as Hispanic.

However, the assumption may no longer hold with the new question. In 1990, Hispanics and non-Hispanics were equally likely to leave the Hispanic item blank (McKenney et al., 1993), implying that Hispanics were *equally represented* among people who responded and those who did not respond to the item in 1990. Recently available evidence from the Content Reinterview Survey for Census 2000 suggests that 25 percent of people who did not respond to the Hispanic item in Census 2000 reported as Hispanic in the reinterview (Singer and Ennis, forthcoming). This suggests that Hispanics were *overrepresented* among nonrespondents to the Hispanic item in this census. The high nonresponse rate (esp. for 1990-style forms), uncertainty about what fraction of nonrespondents to the item are Hispanic, and the possibility that the fraction varies by form type, creates uncertainty about whether there might be differences between forms in the fraction identified as Hispanic, after the data were fully edited and imputed. Any such differences are not measurable using this sample.

Effects of Examples: Detailed Hispanic Reporting. Although there were no apparent questionnaire effects on the fraction reported as Hispanic, there were effects on the reporting of detailed Hispanic origin groups.

It has been suggested that dropping examples from the Hispanic origin question in the Census 2000 mail form may have resulted in a loss of detail in Hispanic reporting. To examine this possibility, Hispanic origins were classified into four categories, as shown in Table 6:

- Groups with *check boxes* (Mexican, Puerto Rican, Cuban) in both 1990- and 2000-style forms;

- Groups listed as *examples* in the 1990- but not the 2000-style form (Argentinian, Colombian, Dominican, Nicaraguan, Salvadoran, Spaniard);
- *All other specific groups* with no check boxes and not listed as examples in either form; and
- *General descriptors*: “Hispanic,” “Latino,” or “Spanish” were written in, rather than a specific group.

Detailed Hispanic Reporting, by Form Type (Table 6)

	2000-style	1990-style	t ₂₀₀₀₋₁₉₉₀
All people identified as Hispanic	100.00%	100.00%	
“Check box groups”: Hispanic groups with separate check boxes in both forms	70.25%	73.23%	-1.37
“Example groups”: listed as examples in 1990-style form but not Census 2000	6.41%	11.16%	-3.58*
All other detailed Hispanic groups	4.20	8.68%	-3.38*
Write-in is general descriptor (“Hispanic” / “Latino” / “Spanish”)	11.90%	1.90%	10.32*
Hispanic, no write-in (or write-in uncodable)	7.25%	5.03%	2.15*
N	5,163	3,091	

*p < .05

The 1990-style form elicited more detailed reports of Hispanic origin than the 2000-style questionnaire for all three categories: Hispanic groups with separate check boxes, those listed as examples in 1990 but not 2000, and the remaining detailed groups. The differences are significant for the latter two categories. Overall, about 93 percent of Hispanics reported a specific group in 1990-style forms, compared with 81 percent who filled out 2000-style forms. In the 2000-style forms, Hispanics tended to describe their ethnicity in general rather than specific terms. About 12 percent gave Hispanic, Latino, or Spanish as their “group,” compared with 2 percent in 1990-style forms.

These results suggest that the examples helped respondents understand that the intent of the question was to elicit a detailed Hispanic origin, and thereby improved reporting of both example groups and non-example groups. However, the reporting differences are likely due to the effects of question wording differences as well as examples. There was a significant difference in reporting for one of the checkbox categories, the wording of which was identical in both forms (“Yes, Mexican, Mexican-Am., Chicano”). About 54 percent of Hispanics checked the Mexican box (or wrote in Mexican) in 2000-style forms, compared to 59 percent in the 1990-

style forms. This difference may result from dropping the word “origin” from the question. A subsequent experiment has confirmed that question wording differences account for most of the difference in detailed Hispanic reporting, although the examples also contributed (Martin, 2003).

Race Reporting. Race reporting was also affected by differences between the questionnaires. Overall, reports of two or more races more than doubled (.82 percent to 2.03 percent) in response to the “mark one or more” instruction, there were significantly more reports of Native Hawaiian and Other Pacific Islander (NHOPI), and significantly fewer reports of “Some other race.” (These results are not shown.) Contrary to what might have been expected, there is little evidence that allowing respondents to report more than one race reduced single race reporting in the five major race categories.

The effects of the questionnaire differences on race reporting by Hispanics were marked. Reporting as White was higher by about 10 percent, while reporting as Some other race was lower by about the same amount, in 2000-style forms, as shown in Table 7. Missing or uncodable responses are excluded, so the distribution approximates the distribution that would be obtained were missing data imputed. The form differences in reporting of Some other race are consistent with prior research, and are probably due to the effects of reversing the order of Hispanic and race items, as well as the new “one or more” option.

Race Distributions for Hispanics, by Form Type (Table 7)

	Form type		t-statistic
	2000-style	1990-style	t ₂₀₀₀₋₁₉₉₀
All people identified as Hispanic	100.00%	100.00%	
White	48.98	39.88	3.23*
Black	2.07	2.32	-.34
American Indian and Alaska Native	1.48	.72	1.61
Asian	.58	.88	-.60
Native Hawaiian and Other Pacific Islander	.01	.15	-1.14
Some other race	39.03	51.47	-4.32*
Two or more races	7.84	4.59	2.88*

*p<.05

Example Effects: Race Reporting³. In the 1990-style form, examples of “other Asian or Pacific Islander” groups were placed in the leftmost column of the matrix, below the race question (see Fig. 5). These examples (Hmong, Fijian, Laotian, Thai, Tongan, Pakistani, Cambodian) were dropped in the 2000-style form. Table 8 shows the fraction who reported in a race example group in 1990- and 2000-style questionnaires. (People for whom the race question was left entirely blank are dropped from the table.)

Percentage Who Reported in a Race “Example Group,” by Form Type (Table 8)

	1990-style	2000-style
Wrote in Hmong, Fijian, Laotian, Thai, Tongan, Pakistani, or Cambodian	.106% (.0351)	.356% (.0606)
Checked or wrote in another race	99.894	99.644
Total	100.000%	100.000%

The table shows a highly significant form difference ($t=3.58$) but its direction is unexpected: there are three times as many reports of the example groups in the 2000-style form, which did not list examples. One would expect the use of examples to be associated with higher, not lower, reporting of example categories.

Table 9 shows the fraction reporting in each specific “example group.” In general, the 2000-style form elicited more reports of both the Asian and the Pacific Islander example groups, although only the overall differences for Asians and for Pacific Islanders are statistically significant at the .05 level. Note that there were no write-ins of the example Pacific Islander groups in 1990-style forms. Clearly, for the purpose of assessing example effects for Pacific Islanders, a larger sample is needed. Nevertheless, the difference is consistent for all the groups, and marginally significant for several ($t \geq 1.645$ is significant at $p < .10$ with a 2-tailed test), despite very small cell sizes.

³This section was not included in Martin (2002).

Percent Writing in Each “Example Group” by Form Type (Table 9)

	1990-style	2000-style	t ₂₀₀₀₋₁₉₉₀
Asian examples	.106% (.0351)	.300% (.0545)	2.99*
Cambodian	.014 (.0139)	.041 (.0164)	1.27
Hmong	.002 (.0023)	.044 (.0243)	1.69†
Pakistani	.029 (.0217)	.095 (.0317)	1.72†
Thai	.051 (.0218)	.054 (.0188)	.09
Lao	.010 (.0097)	.066 (.0276)	1.92†
Pacific Islander examples	0	.057% (.0264)	2.16*
Fijian	0	.019 (.0119)	1.60
Tongan	0	.038 (.0236)	1.61
Another race written in or checked	99.894 (.0351)	99.644 (.0606)	-3.58*
Total	100.000%	100.000%	

**p<.05, 2-tailed test.

†p<.10, 2-tailed test

It is useful to interpret these results in the context of overall questionnaire differences in reporting of major race groups (from Table 5, Martin, 2002):

- There was no difference between questionnaires in the overall fraction reporting an Asian race: 4.04 percent reported as Asian in 2000-style questionnaires, compared to 4.06 percent in 1990-style questionnaires.
- There was a significant questionnaire difference in the overall fraction reporting as Native Hawaiian and Other Pacific Islander: 0.17 percent reported as NHOPI in 2000-style

forms, compared to 0.05 percent in 1990-style forms. Thus, the difference in reporting of example groups is consistent with an overall reporting difference for this group.

Other questionnaire features, such as splitting the Asian and Pacific Islander (API) category into two separate categories, undoubtedly influenced the results for Pacific Islanders. The Pacific Islander category is probably more populated in 2000-style forms because it is easier for Pacific Islanders to report with the Pacific Islander boxes grouped together rather than interspersed among Asian boxes, as they are in the 1990-style form, and with their own “Other Pacific Islander” check box associated with a write-in space (Cf. Figs. 4 and 5). For these reasons, and because of the very small sample size, we cannot draw even tentative inferences about the effects of the Pacific Islander examples.

The evidence appears stronger that the Asian examples may have affected reporting. The greater reporting of Asian example groups in the 2000-style form is not consistent with an overall increase in reporting for Asians as a whole, as is the case for Pacific Islanders. Moreover, the form differences are consistent and statistically significant overall, as well as for three individual example groups (Hmong, Pakistani, Lao). The results suggest, although they do not prove, that the use of Asian examples in the 1990-style questionnaire somehow reduced reporting in the example groups.

The contrast between the effects of examples in the Hispanic origin and race items merits further analysis and research. The examples in the 1990 Hispanic origin question seem to have helped to clarify the intent of the question to collect detailed Hispanic origin, while the examples in the race question did not help reporting and may have adversely affected reporting of Asian example groups. Possibly, the different placement of the examples was a factor. In the 1990-style form, the Hispanic examples were prominently placed above the write-in space, just below the “other Spanish/Hispanic” response option. The race examples were off to the left, below the question and remote from the write-in space. Respondents may not have understood how to interpret the meaning of the arrow that connected them. The physical distance between the examples and the write-in space may have meant that many respondents never saw the examples, while those who did may not have realized they were meant to be associated with a write-in space. The examples may have distracted respondents and disrupted the response process. Perhaps respondents were confused by the label “Other API” for the write-in box in the 1990-style form, which they may not have realized applied to them, so attempted to find some way of reporting their race other than writing in a response. Possibly, respondents in the example groups who found the examples circled them, without writing in a response. Testing these hypotheses and speculations would require examining the forms, which we have not undertaken, or additional experiments. Alternatively, it is possible that features of the questionnaire other than examples somehow explain these results.

In any case, these results suggest the possibility that in some circumstances, examples may interfere with responding in the example groups. More controlled experiments are needed to understand the effects of examples on race and Hispanic reporting, and how questionnaire

features such as their location influence the response process. Meanwhile, considerable caution in their use appears to be warranted.

Conclusions. Additional research is needed into methodological influences on race reporting by Hispanics and non-Hispanics (including experimental research to evaluate mode differences in reporting, which are not explored here but are troubling; see Martin and Gerber, 2003) .

The results raise doubts about the meaningfulness of race data, especially for Hispanics, for whom race reporting is highly vulnerable to methodological influences. Research is needed to develop more robust race measurement methods that are less vulnerable to methodological effects, especially for Hispanics, and to evaluate the effects of examples on race and Hispanic origin reporting.

Coding, pre-editing, editing, and imputation procedures may substantially affect the quality and comparability of race and Hispanic origin data, and their effects are largely hidden. They need to be documented and evaluated in conjunction with questionnaire design changes.

4. Implications and Recommendations

The results of the three experiments share certain themes in common. In this section, we discuss these themes and recommend new avenues for research.

The central lesson of this research is that we must attend to the complex relationships between format and meaning in self administered questionnaires. These factors have measurable effects on the data. Our experiments demonstrate the effects of questionnaire format on performance on branching instructions, item nonresponse on the household count question and coverage of Hispanics, race and ethnicity reporting, and response rates. In addition, the same questionnaire design feature (e.g., examples) may have complex and varying effects, depending on how it is used and integrated into the questionnaire as a whole. Examples apparently helped respondents understand the intent of the Hispanic origin question, and provide more detailed data. But Asian examples in the race question may have actually reduced reporting of the example groups. **Our results provide a caution that seemingly minor questionnaire variations in the design of a questionnaire can have substantial effects on the data.**

We recommend against introducing any questionnaire changes without fully pretesting and field testing them, since they may have complex and unexpected consequences.

We also note that cognitive testing alone is not sufficient to understand the effects of questionnaire design changes on the data. Cognitive testing alone may prove misleading, and must be supplemented by field experiments. As discussed above, some respondents in cognitive

tests, as well as expert reviewers, found the arrows in the Detection method in Experiment A distracting, yet this method resulted in significantly lower error rates.

These results should focus our attention on the importance of the visual aspects of self-administered questionnaires. As the Redline experiment points out, these visual aspects impart meaning in themselves, in that they guide basic questionnaire behavior of respondents. Two factors appear to be at work. One is the *attention* of respondents. There is little value in altering the wording of a question or instructions if the visual format does not draw the respondent's attention. The second is *pattern recognition*, or respondents' perception that certain visual elements of a questionnaire are associated, and the inferences and meaning they draw from the groupings they perceive. By manipulating visual features of the questionnaire, Redline succeeds in reducing the frequency with which respondents answer questions they should have skipped, or skip questions they should have answered. **We recommend that the Census Bureau use the Detection method developed by Redline in mail questionnaires because it results in fewer errors.** The Martin experiment also suggests that visual elements of the design influence respondents' behavior in complex ways, since the effects of examples appear to depend on their placement on the questionnaire (and possibly on other, as yet unidentified, questionnaire design features as well). Gerber attempts a holistic design that graphically integrates the list of residence instructions and the household count box. While we cannot be sure whether the graphical features or the wording changes in her design were responsible for the improvements she obtained, her experiment also reduces errors, supporting its use as well as further research and development of her innovations. **We recommend that the Census Bureau conduct further tests of graphical design and wording changes in the residence instructions, which show promise of improving coverage.**

All three experiments suggest that visual presentation and organization affect respondents' perception, comprehension and performance on self-administered questionnaires. **We recommend further theoretical and empirical questionnaire design research that draws on relevant scientific disciplines to better understand how graphical design features affect the response process.** This includes additional research on the reading of questionnaires. New methodologies, such as eye movement research, are necessary to establish a baseline for how questionnaires are normally read, in order to be able to better understand how questionnaire design features affect the reading and response process. **We also recommend that the Census Bureau add the analysis of errors of commission and omission to its arsenal of data quality measures, as these are more direct measures of navigational performance than item non-response rates or edit changes.** Using measures of data quality that are relevant both to questionnaire designers and to statisticians may improve their communication about data quality.

Some format effects appear to be more relevant for one group than others. Gerber found that the revised residence rules had stronger effects for Hispanics than non-Hispanics in low coverage areas, perhaps because the changes in wording and organization made the question easier to understand and follow. Martin found response rate differences for the low coverage stratum that included more Black and Hispanic households. In both cases, design changes intended to be

“user-friendly” had a bigger effect for respondents who otherwise were less likely to respond or have difficulty with the questionnaire. In addition, questionnaire design differences sometimes affected race reporting by Hispanics and non-Hispanics in opposite ways (see Martin, 2002).

We have not explored the basis for these differences, so cannot address their causes. Possibly, differences in learning and expectations, and training in the form-filling task, underlie them. This suggests that **additional basic research should be carried out to understand how cognitive as well as sociocultural factors affect questionnaire performance.**

The Martin experiment identifies questionnaire effects that confound comparisons of 1990 and 2000 census data. The degree of confounding cannot be inferred directly from the experimental evidence, which is restricted to mail short forms and does not employ fully edited data. However, we can infer that the differences in the design of 1990 and 2000 mail short forms would have resulted in an increase from the 1990 to the 2000 census in Hispanics’ reporting of White race, and a decline in their reporting of detailed Hispanic groups, *in the absence of true change in the racial or ethnic composition or identifications of the population*. The percentage of Hispanics who reported as White (alone) was 51.7 in the 1990 census and 47.9 in Census 2000 (U. S. Census Bureau, 2001). Because the questionnaire effect would have led more Hispanics to report as White, we infer that the decline in White reporting would have been *even larger* had the 2000-style questionnaire not increased Hispanics’ reporting as White, compared to a 1990-style questionnaire. We can also infer that any measured decline from the 1990 to the 2000 census in reporting of detailed Hispanic origins is overstated; the decline would be less if the 2000-style questionnaire had not resulted in less detailed reporting. **The confounding effects of questionnaire differences should be taken into account when comparing 1990 and 2000 census data.**

This panel of the AQE had as its main purpose *calibrating* the measurement properties of the 1990 and 2000 mail short form questionnaires. By replicating the 1990 census form in Census 2000, it is possible to evaluate whether differences in reporting are attributable to the questionnaire, and must be taken into account in interpreting population trends from one census to the next. The results demonstrate that replicating a prior census’s questions in the current census can help shed light on possible errors and reporting differences that otherwise would be the subject of unchecked conjecture and speculation. For example, the AQE data made it possible to understand how changes in the questionnaire caused an unexpected decline in detailed Hispanic reporting between 1990 and 2000 censuses. Without the AQE data, analysts might have been tempted to explain the apparent trend as the result of an increasing pan-Hispanic identification in the Hispanic population, when it appears to be an artifact of the questionnaire design. **We recommend that a similar replication be carried out in 2010, and that the Census 2000 long form be included,** to calibrate the different versions of the long form used in the census in 2000 and in the American Community Survey (ACS) in 2010.

The AQE sample was not large enough to permit us to test design changes separately. Rather, the experiments (particularly the Martin and Gerber experiments) all tested “packages” which

confound the effects of format and content, and which do not permit us to isolate the factors responsible for effects on particular populations. More sophisticated experimental designs might permit future experiments to disentangle these effects. In addition, where coverage is measured, larger reinterview samples are needed, since omissions and erroneous enumerations account for a very small fraction of the entire population. These limitations were implicit in the size and scope of the current AQE. Both for the purposes of calibrating measurements (as in the Martin experiment), and the purpose of measuring small improvements with high precision (as in the Gerber experiment), **we recommend much larger sample sizes and more sophisticated experimental designs for Alternate Questionnaire Experiments of the future.**

Acknowledgment

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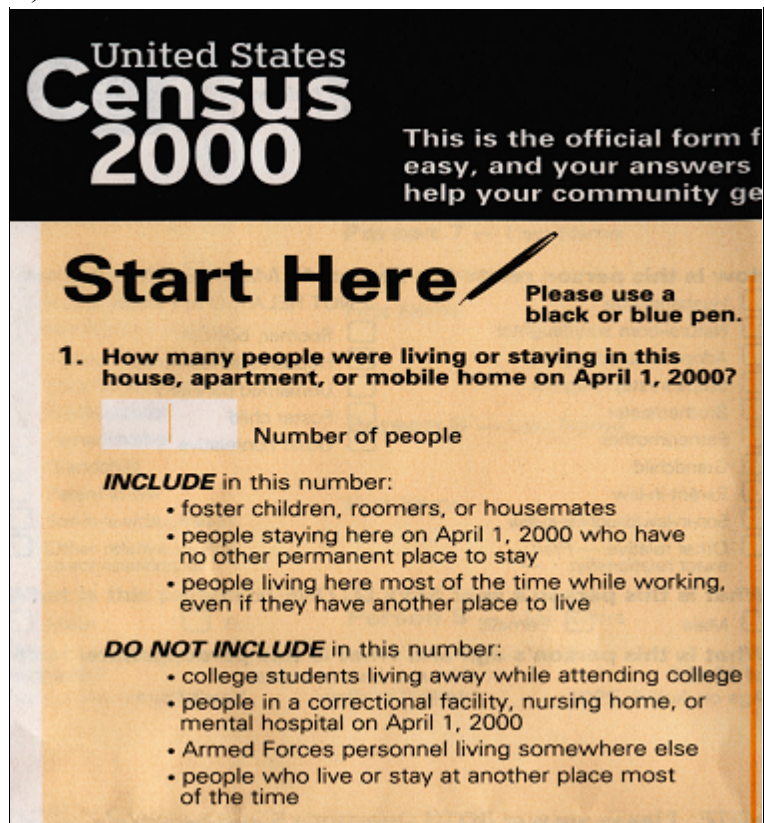
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Illustration of the five branching instruction treatments. (Figure 1)

a. Census 2000 Skip To Instruction	<p>30 a. LAST YEAR, 1999, did this person work at a job or business at any time?</p> <p><input type="checkbox"/> Yes</p> <p><input type="checkbox"/> No → <i>Skip to 31</i></p> <p>b. How many weeks did this person work in 1999?</p>
b. Go To Instruction	<p>30 a. LAST YEAR, 1999, did this person work at a job or business at any time?</p> <p><input type="checkbox"/> Yes</p> <p><input type="checkbox"/> No → <i>Go to 31</i></p> <p>b. How many weeks did this person work in 1999?</p>
c. (Go To) Reverse Print Instruction	<p>30 a. LAST YEAR, 1999, did this person work at a job or business at any time?</p> <p><input type="checkbox"/> Yes</p> <p><input type="checkbox"/> No → Go to 31</p> <p>b. How many weeks did this person work in 1999?</p>
d. (Go To) Prevention Instruction	<p>30 Attention: Remember to check for a "Go to" instruction after you answer the question below.</p> <p>a. LAST YEAR, 1999, did this person work at a job or business at any time?</p> <p>Yes <input type="checkbox"/></p> <p>No <input type="checkbox"/> → Go to 31</p> <p>b. How many weeks did this person work in 1999?</p>
e. (Go To) Detection Instruction	<p>30 a. LAST YEAR, 1999, did this person work at a job or business at any time?</p> <p><input type="checkbox"/> Yes</p> <p>↓ <input type="checkbox"/> No → Go to 31</p> <p>b. (If Yes) How many weeks did this person work in 1999? <i>Count paid vacation, paid sick leave, and military</i></p>


Figure 2. Census 2000 Roster Instructions (Experiment B)



The image shows a portion of the United States Census 2000 Roster Instructions form. The top section has a black background with white text. Below this, the form is yellowed and features a large 'Start Here' heading with a pencil icon. To the right of the heading is a note about using a black or blue pen. The first instruction asks for the number of people living or staying in the household on April 1, 2000. It includes a box for the answer and a list of who to include and exclude from the count.

United States
Census 2000

This is the official form for
easy, and your answers
help your community ge

Start Here  Please use a
black or blue pen.

**1. How many people were living or staying in this
house, apartment, or mobile home on April 1, 2000?**

Number of people

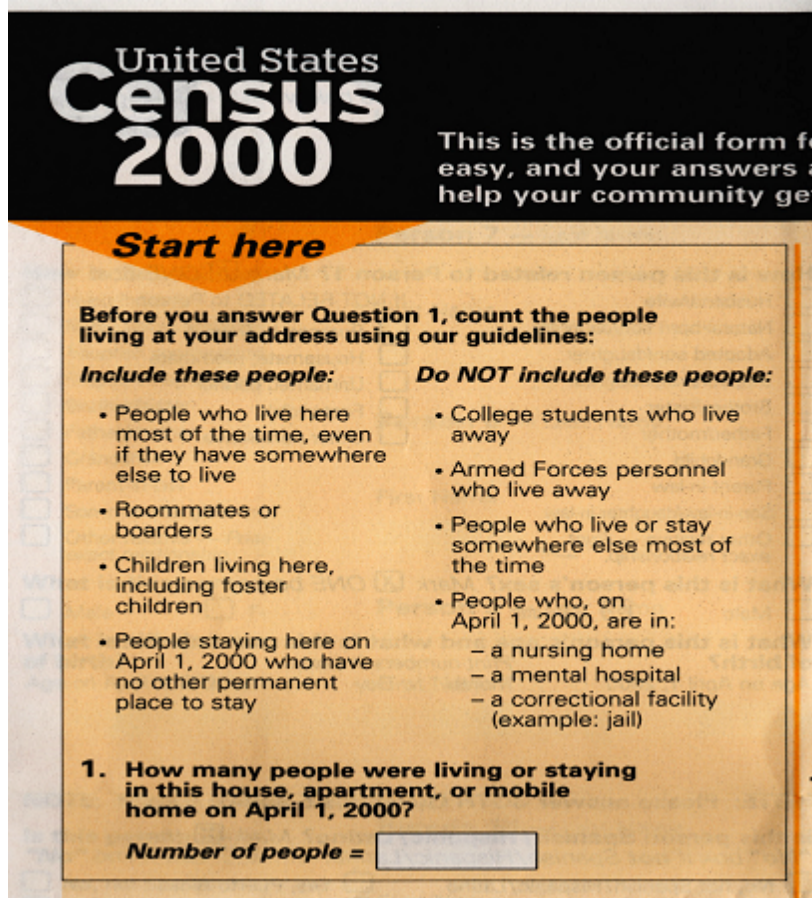
INCLUDE in this number:

- foster children, roomers, or housemates
- people staying here on April 1, 2000 who have no other permanent place to stay
- people living here most of the time while working, even if they have another place to live

DO NOT INCLUDE in this number:

- college students living away while attending college
- people in a correctional facility, nursing home, or mental hospital on April 1, 2000
- Armed Forces personnel living somewhere else
- people who live or stay at another place most of the time

Figure 3. Roster Instructions in Experimental Form (Experiment B)



The image shows a section of the United States Census 2000 form. At the top, the text "United States Census 2000" is displayed in a large, bold font. To the right, a smaller text block states: "This is the official form for easy, and your answers will help your community get...". Below this, a yellow banner with the text "Start here" is visible. The main instruction reads: "Before you answer Question 1, count the people living at your address using our guidelines:". This is followed by two columns of guidelines. The left column, titled "Include these people:", lists four bullet points: "People who live here most of the time, even if they have somewhere else to live", "Roommates or boarders", "Children living here, including foster children", and "People staying here on April 1, 2000 who have no other permanent place to stay". The right column, titled "Do NOT include these people:", lists four bullet points: "College students who live away", "Armed Forces personnel who live away", "People who live or stay somewhere else most of the time", and "People who, on April 1, 2000, are in: - a nursing home - a mental hospital - a correctional facility (example: jail)". Below these guidelines, Question 1 is presented: "1. How many people were living or staying in this house, apartment, or mobile home on April 1, 2000?". The question is followed by the text "Number of people =" and a rectangular box for the answer.

**United States
Census
2000**

This is the official form for easy, and your answers will help your community get...

Start here

Before you answer Question 1, count the people living at your address using our guidelines:

Include these people:	Do NOT include these people:
<ul style="list-style-type: none">• People who live here most of the time, even if they have somewhere else to live• Roommates or boarders• Children living here, including foster children• People staying here on April 1, 2000 who have no other permanent place to stay	<ul style="list-style-type: none">• College students who live away• Armed Forces personnel who live away• People who live or stay somewhere else most of the time• People who, on April 1, 2000, are in:<ul style="list-style-type: none">– a nursing home– a mental hospital– a correctional facility (example: jail)

1. How many people were living or staying in this house, apartment, or mobile home on April 1, 2000?

Number of people =

Figure 4. Census 2000-style Form: Race and Hispanic Questions (Experiment C)

→ **NOTE: Please answer BOTH Questions 5 and 6.**

5. Is this person Spanish/Hispanic/Latino? Mark ☒ the "No" box if *not* Spanish/Hispanic/Latino.

☐ No, not Spanish/Hispanic/Latino ☐ Yes, Puerto Rican
☐ Yes, Mexican, Mexican Am., Chicano ☐ Yes, Cuban
☐ Yes, other Spanish/Hispanic/Latino — *Print group.*

☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐

6. What is this person's race? Mark ☒ *one or more races* to indicate what this person considers himself/herself to be.

☐ White
☐ Black, African Am., or Negro
☐ American Indian or Alaska Native — *Print name of enrolled or principal tribe.*

☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐

☐ Asian Indian ☐ Japanese ☐ Native Hawaiian
☐ Chinese ☐ Korean ☐ Guamanian or Chamorro
☐ Filipino ☐ Vietnamese ☐ Samoan
☐ Other Asian — *Print race.* ☐ Other Pacific Islander — *Print race.*

☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐

☐ Some other race — *Print race.*

☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐

Figure 5. 1990-style Questionnaire: Race and Hispanic Questions (Experiment C)

<p>4. Race Fill ONE circle for the race that the person considers himself/herself to be.</p> <p>If Indian (Amer.), print the name of the enrolled or principal tribe. →</p> <p>If Other Asian or Pacific Islander (API), print one group, for example: Hmong, Fijian, Laotian, Thai, Tongan, Pakistani, Cambodian, and so on. →</p> <p>If Other race, print race. →</p>	<p><input type="radio"/> White <input type="radio"/> Black or Negro <input type="radio"/> Indian (Amer.) (Print the name of the enrolled or principal tribe.) →</p> <p><input type="radio"/> Eskimo <input type="radio"/> Aleut Asian or Pacific Islander (API) <input type="radio"/> Chinese <input type="radio"/> Japanese <input type="radio"/> Filipino <input type="radio"/> Asian Indian <input type="radio"/> Hawaiian <input type="radio"/> Samoan <input type="radio"/> Korean <input type="radio"/> Guamanian <input type="radio"/> Vietnamese <input type="radio"/> Other API →</p> <p><input type="radio"/> Other race (Print race) →</p>	<p><input type="radio"/> White <input type="radio"/> Black or Negro <input type="radio"/> Indian (Amer.) (Print the name of the enrolled or principal tribe.) →</p> <p><input type="radio"/> Eskimo <input type="radio"/> Aleut Asian or Pacific Islander (API) <input type="radio"/> Chinese <input type="radio"/> Japanese <input type="radio"/> Filipino <input type="radio"/> Asian Indian <input type="radio"/> Hawaiian <input type="radio"/> Samoan <input type="radio"/> Korean <input type="radio"/> Guamanian <input type="radio"/> Vietnamese <input type="radio"/> Other API →</p> <p><input type="radio"/> Other race (Print race) →</p>																																																																																																																																																																																																																																																																																																																																
<p>5. Age and year of birth</p> <p>a. Print each person's age at last birthday. Fill in the matching circle below each box.</p> <p>b. Print each person's year of birth and fill the matching circle below each box.</p>	<p>a. Age b. Year of birth</p> <table border="1"> <tr> <td>0 <input type="radio"/></td><td>0 <input type="radio"/></td><td>0 <input type="radio"/></td><td>1 <input type="radio"/></td><td>0 <input type="radio"/></td><td>0 <input type="radio"/></td><td>0 <input type="radio"/></td><td>0 <input type="radio"/></td><td>0 <input type="radio"/></td><td>1 <input type="radio"/></td><td>0 <input type="radio"/></td><td>0 <input type="radio"/></td><td>0 <input type="radio"/></td><td>0 <input type="radio"/></td><td>0 <input type="radio"/></td><td>0 <input type="radio"/></td> </tr> <tr> <td>1 <input type="radio"/></td><td>1 <input type="radio"/></td><td>1 <input type="radio"/></td><td>2 <input type="radio"/></td><td>0 <input type="radio"/></td><td>8 <input type="radio"/></td><td>1 <input type="radio"/></td><td>0 <input type="radio"/></td><td>1 <input type="radio"/></td><td>2 <input type="radio"/></td><td>0 <input type="radio"/></td><td>8 <input type="radio"/></td><td>1 <input type="radio"/></td><td>0 <input 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<p>6. Is this person of Spanish/Hispanic origin? Fill ONE circle for each person.</p> <p>If Yes, other Spanish/Hispanic, print one group. →</p>	<p><input type="radio"/> No (not Spanish/Hispanic) <input type="radio"/> Yes, Mexican, Mexican-Am., Chicano <input type="radio"/> Yes, Puerto Rican <input type="radio"/> Yes, Cuban <input type="radio"/> Yes, other Spanish/Hispanic (Print one group, for example, Argentinean, Colombian, Dominican, Nicaraguan, Salvadoran, Spaniard, and so on.) →</p>	<p><input type="radio"/> No (not Spanish/Hispanic) <input type="radio"/> Yes, Mexican, Mexican-Am., Chicano <input type="radio"/> Yes, Puerto Rican <input type="radio"/> Yes, Cuban <input type="radio"/> Yes, other Spanish/Hispanic (Print one group, for example, Argentinean, Colombian, Dominican, Nicaraguan, Salvadoran, Spaniard, and so on.) →</p>																																																																																																																																																																																																																																																																																																																																